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other hand, just as in the solar system the planetary orbits are at the same time of small eccentricity and systematically disposed near one plane (which is at a high inclination to the Galaxy), while comets have orbits of high eccentricity with planes in miscellaneous orientations,—so there may be a real difference between double star orbits of low and of high eccentricity.

19. It seems worth while to form a new table corresponding to Table II. for the 26 orbits retained in Table VIII.

TABLE IX.

Similar to Table II., but excluding orbits of small size ($a < 0''\cdot 5$) *and high eccentricity* ($e > 0\cdot 51$).

i	Star's No.	Gal. Lat.	*i*	Star's No.	Gal. Lat.	*i*	Star's No.	Gal. Lat.
11	15	+26°	56	20	+70°	75	17	+44°
14	43	+38	58	48	+10	75	42	+40
34	51	−19	61	54	−17	77	4	−87
37	44	+ 1	64	46	+25	81	45	+17
43	5	− 6	66	9	−13	84	37	+50
44	1	− 4	67	50	−16	90	27	+82
47	57	− 8	69	53	+10			
49	23	+53	69	11	−37			
52	34	+56	72	2	+16			
56	59	−35	73	55	−30			

Ancient Eclipses. By P. H. Cowell, F.R.S.

As my whole argument from the ancient lunar eclipses is—"If I have placed a wrong interpretation upon the records of solar eclipses, how is it that the results so obtained agree so well with the lunar eclipses?" I am, of course, perfectly satisfied with such sentences as these in Mr Nevill's recent papers:—

"During past years in my researches I have repeatedly found evidence of an unexplained apparent secular acceleration in the motion of the Moon's argument of latitude . . . ." (*M.N.*, lxvii. p. 17).

"These early eclipse observations . . . . are certainly not inconsistent with Mr Cowell's conclusion that the Moon's argument of latitude requires an increased secular acceleration" (p. 13).

In my view the lunar eclipses by themselves do not amount to a proof, and I do not ask Mr Nevill to say more than he has said.

Mr Nevill, however, attaches more weight than I do to the times of lunar eclipses. From the times the mean elongation is

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deduced, whereas the argument of latitude is found from the magnitudes. For the mean elongation, Mr Nevill deduces a secular term 7″·4 from the Ptolemaic eclipses and 6″·2 from the Arabian eclipses. I regard the agreement of these numbers with the 6″·8 that I obtained from solar eclipses as most satisfactory. Mr Nevill (p. 10) regards 6″·8 as inconsistent with both 6″·2 and 7″·4. Apparently, upon the inconsistency of 6″·2 with 7″·4, he bases his supposition of unknown long-period terms. But he has not shown that the 6″·2 and 7″·4 are subject to such small probable errors as to negative the possibility that the discordance is accidental. My view, of course, is that 6″·8 is nearly the true value.

It is on the solar eclipses that I rely. The divergence between Mr Nevill's views and my own in this case arises largely from differences of numerical calculation, and the first thing to be done is to remove those differences. The following table gives for several solar eclipses (1) my residual, which is approximately the least distance between the centres of the Sun and Moon as seen from the place assumed in the calculations; (2) the quantity which Mr Nevill calls "Cowell's $d\phi$."

Date.	Residual. See *M.N.*, lxvi. 531.	$d\phi$ See *M.N.*, lxvi. 414, 416.
	″	°
− 309	− 177	− 0·9
− 399	+ 14	+ 3·1
− 403	− 525	− 2·4
− 430	+ 34	+ 0·1
− 602	+ 46	− 0·6
− 647	+ 8*	+ 0·5
− 660	+ 163	+ 6·0
− 678	+ 115	+ 4·8
− 762	− 42	− 0·8
− 1062	+ 29	+ 0·8
− 1123	− 697	− 2·1
− 600	+ 429	+ 2·0
− 708	+ 211	− 3·0
− 180	+ 27	+ 0·9
− 187	+ 456	+ 1·9
− 299	− 157	− 3·3

The relationship between my residual and Mr Nevill's $d\phi$ is a complicated one; the following test will, however, serve to detect large errors:—"Divide $d\phi$ by 60 and compare with the residual, allowing 100″, partly because the divisor 60 is only approximate

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* From *M.N.*, lxv. p. 867.

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and partly on account of approximations used in obtaining the residual, and possibly other approximations used in obtaining $d\phi$."

It will be noted that this is a very rough test. My point is that certain formulæ will give residuals of less than 50″ for several eclipses, and in applying the above test I allow 100″; hence discrepancies in arithmetic that are important for my argument may slip through, undiscovered by the test, which only detects the still larger discrepancies.

The test, however, brings to light discrepancies in the eclipses of −309, −399, −403, −660, −678, −1123, −600, −708, −187. To this list −1116 would also have been added if Mr Nevill (*M.N.*, lxvii. p. 15) had not corrected his result as given on *M.N.*, lxvi. p. 414.

Assuming for the moment that my figures are correct, it is evident that Mr Nevill has not appreciated the superior precision of solar eclipses. Lunar eclipses are liable to errors of a tenth of a magnitude or 200″, but solar eclipses appear to be only liable to uncertainties of 50″, a little greater than the maximum difference of semidiameter.

If, on the other hand, Mr Nevill's figures are correct, solar eclipses exhibit residuals as large as lunar eclipses, and the case for an unexplained acceleration in the Moon's argument of latitude ceases to amount to anything like certainty.

Now it would be only natural that Mr Nevill and I should each of us believe that the discrepancies exhibited above are due to the blunders in computation of the other. What I now ask of Mr Nevill is that he should put me into the same position with regard to his computations as that into which I have already put him with regard to mine. I have set down every formula that I have used. In my papers on the eclipses of +1030, +1239, and +1241, it can be seen to what extent the formulæ used for ancient eclipses are approximate. On *M.N.*, lxvi. pp. 533–536, a full calculation is given for one eclipse. In all cases numerical results for a great number of intermediate stages are given.

But, in Mr Nevill's computations, I do not know whether he has calculated his own places for the Sun and Moon, or used Oppolzer's, with suitable modifications. In either case I do not know what inequalities he has retained. Further, I should like to see the numerical places obtained in the various cases, so that I can tell whether the discrepancies between our results arise in the geocentric places or parallactic part of the calculations. Again, on *M.N.*, lxvii. p. 15, Mr Nevill says: "The changed path of the curve of central totality can be deduced from the comparison of the new positions of the beginning, middle, and the end of eclipse with those given by Oppolzer." What path of the curve of central totality underlies the changes deduced by Mr Nevill? The paths published by Oppolzer are not sufficiently accurate, but Mr Nevill has not stated by what formulæ he has calculated his own paths. All the information for which I ask could be supplied if Mr Nevill, in addition to some intermediate results for all eclipses, would publish at full length and with suitable explanations the entire calculation for one eclipse. If he will do this, I should be glad if he will select the eclipse of − 1123. This is a very important one, for his figures on *M.N.*, lxvi. p. 414, indicate that the theoretical accelerations (Mr Nevill's B) make this eclipse total near Babylon. My figures on *M.N.*, lxvi. p. 532, contradict this conclusion.

As the foregoing remarks have brought me to the eclipse of Babylon, I may add—

1. Mr King originally indicated the second half of the eleventh century B.C. as the probable date. He has recently discussed this eclipse very fully in the first volume of *Chronicles concerning Early Babylonian Kings* (Luzac & Co., 1907).

2. According to my calculations, at no date in the summer months of either the eleventh or twelfth centuries B.C. except − 1062 July 31 could the Babylonians have seen any phenomenon answering to the words of the inscription quoted on *M.N.*, lxv. p. 861.

3. Mr King, though he dislikes the equation, Sivan 26 = July 31, does not reject it. I see no alternative to accepting it.

The Perturbations of Halley's Comet. By P. H. Cowell and A. C. D. Crommelin.

The action of Saturn is here considered. The calculations are similar in character to those for the action of Jupiter published on pp. 386–411.

The differential equations given on p. 386 are again applicable. In the value of e_x on that page the square root should be omitted, so that we have

$$e_x = -(1 - e^2) \sin u \cos u.$$

Table I., pp. 387–390, is again applicable. The tabulated values of e_x are correct.

The $V = V_3 + V_4$ method explained on p. 391 is again introduced at $u = 90°$ and dropped at $u = 270°$. V_4 would be the disturbing function if the motion were referred to the centre of gravity of the Sun and disturbing planet. Hence $V_3 = V - V_4$ is the change in the disturbing function that would arise from a displacement of the origin of co-ordinates. This consideration gave us the assurance, *a priori*, that the differential equations depending on V_3 would be integrable.

The positions of Saturn have been taken from a table with argument g' mean anomaly of Saturn similar to Table II., pp. 394–401. The table is not printed here, but it is based upon the following assumptions and formulæ.